Testing the augmented Solow model*

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Abstract

This paper applies robustness ideas from the modern statistics literature to the study of the augmented Solow model. It also tests the model in other dimensions, including sensitivity to measurement error. The main findings are that the speed of conditional convergence is highly uncertain, that technology parameters obtained from the augmented Solow model cannot be trusted, and that the model does not work well when attention is restricted to either the OECD or developing countries. Not only that, the equation for steady state human capital is rejected by the data.

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This paper examines the adequacy of the augmented Solow model for explaining international variation in the standard of living. In particular, is technology usefully described by a common Cobb-Douglas production function, which takes human capital as one of its inputs? Mankiw, Romer and Weil (1992), MRW, marshalled some impressive evidence in favour of this version of the Solow-Swan growth model. Mankiw (1995) argues strongly, partly based on this evidence, that the augmented Solow model can account for the known stylized facts about growth.

In this paper I will suggest that one of these stylized facts - conditional convergence at a rate of 2% a year - is dubious. So too is the assumption of a common production function. The augmented Solow model does not do particularly well when one concentrates attention on developing countries, and does even less well at explaining variation within the OECD. This suggests that the assumption of a common Cobb-Douglas function does not work. The results for the world sample are only good because of wide differences between the OECD and developing countries.

In the next section, I draw out the importance of MRW's results in more detail. In section 2, I see how well the augmented Solow model explains international differences in per capita income. In section 3, I turn to the growth regressions, and the much debated question of conditional convergence. I show that the conditional convergence result, at least as highlighted by Mankiw, Romer and Weil, may be entirely driven by measurement error. Work in section 4 supports the claim of MRW that returns to accumulable factors are diminishing. Section 5 exposes one failing of the augmented Solow model: the implied equation for steady state human capital is rejected by the data. Finally, section 6 concludes.

1 The importance of MRW

Few empirical papers on economic growth have been as influential as that of Mankiw, Romer and Weil. It has been much cited and discussed, and subsequent researchers have often chosen to build on their work.[1] Several authors, including Solow himself, have cited the paper as support for diminishing returns to accumulable factors, rather than the *Ak* formulations that have been popular in the literature. Yet, despite this interest, it remains the case that the robustness of the original results has not been properly tested.

It is not difficult to see the appeal of the MRW approach. Unusually among research on cross-section growth regressions, their paper first derives an equation from theory, and then tests an implied restriction. The framework is simple and elegant, and the results are startling. Apparently, it is possible to explain 80% of the international variation in per capita income using just three variables - the investment ratio, population growth, and a crude proxy for investment in human capital. In the words of the authors, 'an augmented Solow model that includes accumulation of human as well as physical capital provides an excellent description of the cross-country data'.

The MRW regressions assumes that countries are in their steady states, that investment rates averaged over 25 years are good proxies for the determinants of those steady states, and that countries share a common production technology. In suggesting that the data strongly support this model, did Mankiw, Romer and Weil get it right? At one level, they clearly did not. Few people are likely to believe that countries differing widely in culture and institutions share exactly the same production technology, or that those

[1] Among them are Atje and Jovanovic (1993), Cohen (1992, 1993), Durlauf and Johnson (1992), Helliwell (1992), Holtz-Eakin (1993), Knight et al. (1993), Knowles and Owen (1995), Lichtenberg (1992).

countries were all in their steady state in 1985. We know that technical efficiency varies within countries, so it is unlikely to be the same across the world. It would be easy to pick out several countries for which the augmented Cobb Douglas technology is a poor fit, but rather less easy to gain anything useful from the exercise. Instead, this paper is in the spirit of Box (1979): 'all models are wrong, but some are useful'. What we want to know is whether the central assumptions are useful simplifications, or highly misleading.

A recent paper by Canning et al. (1995) has similar aims to this one. They fit a production function with an error-correction mechanism to each country that has time series data on physical capital and human capital stocks. Using these estimates, they strongly reject the hypothesis of a common Cobb-Douglas production function and a common rate of technical progress. Instead, production relations fundamentally differ across countries even in the long run. As discussed above, this is not a surprising result. A better interpretation of growth theories is that they concern parameter averages, and in particular whether returns to accumulable factors are diminishing. When Canning et al. use this viewpoint to assess their results, the Ak formulation is rejected in favour of the augmented Solow model. The approach used here is complementary to that of Canning et al. (1995). Unlike them, I concentrate on a parsimonious representation of cross-country growth experience, one which sacrifices econometric rigour but allows researchers to easily investigate issues like growth and policy across countries.

Testing the augmented Solow model is relevant for at least two central policy questions. MRW's work implies that governments should usually concentrate on raising investment in physical and human capital.[2] An alternative

[2] Although note that since the returns to 'broad capital' (physical and human capital taken together) are found to be diminishing, the paper appears to rule out the Ak strand of endogenous growth theory beginning with Romer (1986), King and Rebelo (1990) and

view is that technology differs widely across countries, so policies to encourage technology transfer need to be better understood. These policies could include not only human capital investment, but also institutions to support licensing arrangements, and more general ways of learning about ideas.

The debate about whether countries share a common technology is sometimes regarded as a semantic one, since it can turn on the favoured definition of human capital. Certainly in models of the Lucas (1988) type there is little useful distinction between technical knowledge as a set of blueprints, and knowledge as a set of skills acquired by workers. In empirical work, given the available measures of human capital, there is a much clearer separation to be made. Variables like average years of schooling in the labour force clearly represent something different from the technical efficiency with which inputs are turned into goods. Efficiency could easily differ across countries even if schooling does not.

It is sometimes argued that the MRW results are uninteresting because they cannot possibly be right. In practice we know technical efficiency can vary widely within national industries, let alone countries. This criticism appears a strong one, but to some extent it misses the point. If it really is possible to explain 80% of the variation in per capita income assuming a common technology and using variables that are badly measured, there is a strong case for believing that international differences in technology, though not negligible, are unlikely to be as important as often thought. This question is at the heart of the debate between Mankiw (1995) and his discussants.

There is a second important policy issue connected to MRW's work: the role of physical capital investment in growth. The Solow model's importance lies in its implication that investment has only a level effect on per capita

Rebelo (1991).

income. However, once one augments the model with human capital, this level effect can be much larger than previously thought. Following MRW, start with the textbook Solow model, in standard notation:

$$\begin{aligned} Y &= K^{\alpha}(AL)^{1-\alpha} \\ L &= L(0)e^{nt} \\ A &= A(0)e^{gt} \end{aligned} \tag{1}$$

Define the stock of capital per efficiency unit of labour, $k = K/AL$, and similarly for output $y = Y/AL$. The evolution of capital is given by

$$\begin{aligned} \dot{k} &= \left(\frac{I}{Y}\right) y - (n+g+\delta)k \\ &= \left(\frac{I}{Y}\right) k^{\alpha} - (n+g+\delta)k \end{aligned} \tag{2}$$

where δ is the rate of depreciation. Solving this equation for steady state capital (by setting $\dot{k} = 0$), substituting this into the production function, and taking logs, gives steady state income per capita as

$$\ln\left[\frac{Y^*}{L}\right] = \ln A(0) + gt + \frac{\alpha}{1-\alpha}\ln(\frac{I}{Y}) - \frac{\alpha}{1-\alpha}\ln(n+g+\delta) \tag{3}$$

This gives the elasticity of income per capita with respect to the investment ratio as $\frac{\alpha}{1-\alpha}$. Taking a rule-of-thumb value of α, 1/3, the elasticity is 1/2, so doubling the investment ratio will raise per capita income by 50%. However, the effect is much larger if we include human capital in the production function, and suggest that human capital evolves in the same way as physical capital. To see this, change the production function to

$$Y = K^{\alpha}H^{\beta}(AL)^{1-\alpha-\beta} \tag{4}$$

Define h as human capital per efficiency unit of labour, h=H/AL, and assume that it evolves as

$$\dot{h} = s_h y - (n+g+\delta)h \tag{5}$$

By the same method as before, this gives

$$\ln\left[\frac{Y^*}{L}\right] = \ln A(0) + gt - \frac{\alpha+\beta}{1-\alpha-\beta}\ln(n+g+\delta) + \frac{\alpha}{1-\alpha-\beta}\ln(\frac{I}{Y}) + \frac{\beta}{1-\alpha-\beta}\ln(s_h) \quad (6)$$

The important thing to note is the elasticity on physical capital investment, which is now rather larger. If α is again 1/3, and β takes a similar value, the output-investment elasticity will be 1. Doubling investment will double per capita income. As MRW point out, because a higher rate of physical capital accumulation leads to higher steady state income, it also leads to a higher steady state level of human capital. The presence of human capital accumulation reinforces the impact of physical capital investment.

2 Explaining 1985 per capita incomes

In this section, I examine the adequacy of the augmented Solow model in explaining international variation in per capita income. The findings of Mankiw, Romer and Weil are easy to replicate; some useful summary statistics for their regressions are presented in Table 1. I also recalculated the standard errors using the jackknife heteroscedasticity-consistent estimator studied by MacKinnon and White (1985). The jackknife HCSEs are little different from the ordinary SEs presented by MRW, indicating that the disturbances are probably homoscedastic, or nearly so. The exception to this is the OECD sample, in which the coefficient on the schooling variable is no longer significant. There is little evidence of heteroscedasticity in any of the regressions using a formal test based on White (1980), although this is likely to have low power.

There is some evidence of mis-specification when the model is applied to the large non-oil sample of 98 countries, since the Ramsey RESET test statistic is highly significant. This indicates that the linear specification may be too simple, or possibly that outliers are present. Other diagnostic tests are generally favourable to the model. A test for functional mis-specification was favourable in all three regressions. As noted by MRW, the theoretical restriction that the coefficients sum to zero is not rejected.

Table 1

All three samples used by MRW include the OECD countries. Grossman and Helpman (1994) argue that the MRW results depend mainly on differences in schooling and population growth between rich and poor countries. If MRW's model is a good one, it should be capable of explaining per capita income when the sample is restricted to developing countries and NICs. In Table 2, results are presented for the MRW income regression when the OECD is excluded from the sample.

Table 2

In some respects, the model continues to perform well (note the high R^2) and the point estimates for investment and schooling are little different from those when the OECD is included. However, the coefficient on the population growth variable is no longer precisely determined. Wald tests of the restriction that the coefficients sum to zero reject at the 5% level. Either the Cobb-Douglas technology is too simple, or perhaps these countries are a long way from their steady states.

One interesting point about these regressions is that the point estimates of the constant term are higher when the OECD is excluded.[3] Remember that this term represents $\ln A(0) + gt$ if the augmented Solow model is correct. Thus these results suggest that either developing countries started with higher initial levels of efficiency - which seems unlikely - or that TFP growth was relatively fast for at least some developing countries. If developing countries began the period with relatively inefficient economies, as seems likely, this reinforces the finding that their TFP growth has been faster. It would be a mistake to read too much into these results, because the standard errors on the constant terms are relatively high. However, we have some indication that the assumption of common technical progress is incorrect.

2.1 Sample sensitivity

The Mankiw, Romer and Weil results could be an illusion. In a cross-section of 100 countries, it is quite possible that the results are heavily influenced by a small group of countries, perhaps by just one or two important outliers. There is little point in carrying out a cross-section regression unless one checks sensitivity to the choice of sample. Here, the results excluding the OECD countries perhaps indicate that a few observations within the OECD are driving the overall findings. This section addresses the question using robust regression, for which a good introduction is that by Rousseeuw and Leroy (1987).

A more obvious approach is to use single-case diagnostics, as discussed in Belsley, Kuh and Welsch (1980). Cook's distance measure, the studentized residuals and DFITS fall into this category. Although easy to implement and interpret, the disadvantages of these diagnostics are increasingly well known

[3]The constant term for the non-oil sample, including the OECD, is 6.84, with a HCSE of 1.03. When the OECD is excluded, the estimate of the constant is 11.5, with a HCSE of 1.98.

in the statistics literature. They may fail to pick up a group of outliers or leverage points (the 'masking effect') or wrongly identify some observations as unrepresentative, due to the presence of others (the 'swamping effect').

A more sophisticated way of exploring the data is provided by robust regression. There are two methods which are easily available and highly resistant to unrepresentative observations: least median of squares (LMS) and least trimmed squares (LTS). The advantage of these methods is that unrepresentative observations, perhaps drawn from a different regime, will be identified and downweighted in fitting the regression line. A corollary is that these methods are highly robust to non-normal disturbances, unlike OLS. The exact procedure I use is discussed in more detail in the appendix.

The identification and exclusion of possible outliers is likely to be viewed with suspicion in some quarters. Perhaps it is often possible to get a desired result simply by playing around with the sample long enough, and omitting arbitrary observations; so the choice of sample should be left alone. This view is badly misguided. It is difficult to justify results which depend on the inclusion of just one or two countries. The technique used here should ensure that the results are robust to the choice of sample. This rather begs some questions; one may be excluding precisely the exceptions that disprove a theory. Yet one should be reluctant to abandon a theory that fits, say, 95 observations well and 5 badly, especially when explaining economic growth.

When observations are excluded from this paper's regressions as a result of LTS estimation, it is often easy to see why they might be unusual. Hong Kong and Singapore, with unusually high investment rates and unusually high growth, are often identified as somehow unrepresentative. At the other end of the scale, Zambia is picked out, with its negative per capita growth over 1960-85. A more thorough study than this one would seek out reasons why these countries do not cohere with the rest of the sample, and perhaps use the information gained to improve the model. Here, I concentrate instead on characterising the most coherent part of the sample, and only note the potential for useful further work.

Table 3

The RWLS results for the five samples are presented in Table 3. It is clear from these regressions that the findings for MRW's largest data set (98 non-oil producing countries) are reasonably robust to the exact choice of sample. Discarding those observations which are highly unrepresentative does not have a large influence on the results. This is also true of the intermediate sample.

Unfortunately, the same cannot be said when attention is restricted to the OECD countries. Use of LTS immediately identifies Greece, Portugal and Turkey as unrepresentative observations. Fitting a line to the most coherent part of the data leaves these countries with large residuals. Their effect on the OLS estimates is a striking demonstration of sample sensitivity. When just Portugal and Turkey are removed from the OECD sample, the R^2 falls from 0.35 to 0.02. It appears that, when one concentrates on the most coherent part of the OECD, the augmented Solow model in this form has no explanatory power. Given that there is little variation in population growth rates and schooling across the OECD, this is perhaps not surprising. It is harder to explain the finding that investment in physical capital can explain little of the difference in per capita incomes. A likely reason is that, given the length of time it takes countries to approach their steady state, investment rates should be averaged over a longer period than 1960-85.

It is also clear from Table 3 that omitting the unrepresentative observations in the non-OECD samples does not improve the results. The augmented Solow model does not do particularly well as an explanation of per capita income within developing countries, and does extremely badly within the OECD. It works best as an explanation of the variation between the two groups.

One reason to be suspicious of these results is suggested by De Long and Summers (1994). They argue that the OECD is a political grouping, not an economic one, and hence is relatively unsuitable for analysis. Although there may be something in this argument, to obtain my results I have excluded Greece, Portugal and Turkey. The remaining sample is relatively homogeneous, since it is essentially the richer industrialised countries of Western Europe together with the US, Canada and Australia. If the augmented Solow model is correct, it is hard to see why it should have so little explanatory power for this group of countries.

We can investigate another way of splitting the sample, by initial per capita income. Durlauf and Johnson (1992) split the MRW sample using 1960 income and literacy rates, and found evidence that technology parameters varied across the samples, suggesting that the assumption of a common technology is a poor one. However, in their samples, the results may be driven by just one or two observations. For instance, Botswana, with high investment and unusually fast growth, can have a large distorting effect on any regression that includes it.

Here, I divide the sample into four quartiles, from 1 (the poorest 25% of countries in 1960) to 4 (the richest 25%). Outliers are dropped from each quartile using the same procedure as before; this leaves 18-20 observations in each sample. The results are shown in Table 4.

Table 4

In some ways the augmented Solow model stands up rather well. It has high explanatory power within each quartile, with R^2's around 0.6, or 0.5 when outliers are included. The adding-up restriction on the coefficients is rejected in only one sample. However, it is clear that the coefficient on $\ln(n+g+\delta)$ is not precisely estimated. In one sample, the second quartile, it is a long way from the value predicted by the model. This pattern was also found in the non-OECD samples considered earlier. It seems that the relation between per capita income and population growth is not properly captured by the augmented Solow model - as one might expect, given the complexities of the demographic transition. An alternative explanation is that rates of technical progress (g) and capital depreciation (δ) vary across countries.

The constant terms, though imprecisely determined, vary little across the four samples. This supports MRW's belief in the constancy of technical progress across countries, unless low initial efficiency in the developing world has been offset by relatively fast subsequent growth. As for the underlying technology, the parameters do appear to vary widely, but are imprecisely determined in these smaller samples. We should be careful about drawing conclusions regarding technology parameters, because measurement error is likely to create large biases. I will now investigate this problem in more detail.

2.2 Measurement error

Measurement error will usually be a serious problem in cross-section growth regressions. Data on observed variables is often poor and, for others, rather crude proxies are used. So far, the 'new growth evidence' has not properly addressed this issue, with one or two exceptions. The approach implicit in MRW, and nearly all other studies, is that the problem can be ignored, since good results can apparently be obtained even in its presence. This ignores the fact that, with several badly measured variables, coefficients can be biased away from zero as well as towards it. Inference should be far more careful given the likelihood of severe measurement error.[4]

If we are willing to make the classical errors-in-variables assumptions[5], we can use reverse regression to obtain information about sensitivity to measurement error. The modern treatment is due to Klepper and Leamer (1984), and their diagnostics have been extended by Klepper (1988), Klepper and Stapleton (1987) and Klepper et al. (1993). These diagnostics exploit the fact that the covariance matrices of the true variables and the disturbances in the true model, and the covariance matrix of the measurement errors, must all be positive semidefinite. These conditions, sometimes together with further restrictions, allow bounds to be placed on the value of the true coefficients. The diagnostics abstract from sampling error, but since this is often small in relation to possible measurement error biases, this tends to be a minor omission in practice.

One problem in using this technique is that, given sufficient measurement

[4]It has been argued to me that since growth researchers are all working with the Summers and Heston data set, measurement error is not a problem, because it will affect everyone's results. I find it hard to see what we gain by having everyone arrive at the same, but wrong, result.

[5]Classically, measurement errors are assumed to be orthogonal to each other and to the unobserved true regressors.

error, it may be that exact collinearity of the true regressors cannot be ruled out. In this case the coefficients are arbitrary and it is impossible to say anything about even their signs. However, we can overcome this by using educated guesses about the likely extent of measurement error. To do this we need to introduce the concept of R^{*2}, the value the R^2 would take if all variables were correctly measured. By imposing a sufficiently low bound, R, on R^{*2} it is possible to obtain bounds on the set of point estimates. Say our R bound is 0.90. Then we are assuming that our model would not explain more than 90% of the variation in per capita income even if all the variables were correctly measured. If we choose a lower value of R, nearer the actual R^2, we are implicitly making a more optimistic assumption about the likely extent of measurement error in our particular data set.

Applying reverse regression to the MRW regressions gives rather wide bounds on the technology parameters. For instance, for the non-oil sample and an R bound of 0.9 (compared to an actual R^2 of about 0.8) then the capital share ranges between 0.1 and 0.6, and the human capital share between 0.1 and 0.5.[6] To try and narrow these bounds, I carried out the reverse regression imposing the restriction that the coefficients sum to zero. I also experimented with a lower R bound of 0.85. We can use the d-statistics described in Klepper (1988) to give some idea of the optimism involved. If the R^2 of the model with correctly measured variables is to lie below 0.85, this must mean that less than 15% of the variation in the variable $\ln(SCHOOL) - \ln(n + g + \delta)$ is due to measurement error, using the d-statistic for this variable. Given the imperfection of SCHOOL as a proxy for the human capital investment rate, the mismeasurement of population growth, and the imputation of common growth and depreciation rates, we are clearly making rather an optimistic

[6] Note that, as in Klepper and Leamer, these bounds do not allow for sampling error, which would make them even wider.

assumption about the likely extent of measurement error.

Yet even with this degree of optimism, the technology parameters α and β can lie anywhere within (0.20, 0.49) and (0.15, 0.38) respectively. For the intermediate sample, which omits the countries with the worst data, the bounds are still wide at (0.18, 0.47) and (0.17, 0.41) respectively. It is clearly a mistake to draw detailed conclusions about technology parameters from cross-section regressions of this kind.

The Klepper-Leamer results, and hence the analysis above, are based on the classical errors-in-variables assumptions. In practice, measurement errors may well be correlated with the regressors. In this case, even error in the dependent variable is problematic. For instance, if a country was away from its steady state in 1985, then observed per capita output measures the steady state value with error. These departures from steady state could well be correlated with the regressors.

In particular, MRW argue that steady state departures are particularly important for the OECD, perhaps as a result of World War II. Alternatively, one could argue that countries which have experienced civil wars, or have had a large fall in output for other reasons, are likely to have been a long way from their steady state in 1985. The probability of these events may again be correlated with the regressors. Although there is no solution to the general problem, this particular one can be alleviated by turning to MRW's cross-section growth regressions, which explicitly allow departures from the steady state. These regressions are considered in more detail in the next section.

3 Explaining growth, 1960-1985

There are two sorts of regressions in MRW: the first set seeks to explain the 1985 level of per capita income, the second set the change in income between 1960 and 1985. The equation to estimate is obtained by using (6) and approximating around the steady state. Substituting for y^* gives:

$$\begin{aligned} \ln y(t) - \ln y(0) &= \theta\frac{\alpha}{1-\alpha-\beta}\ln(\frac{I}{Y}) + \theta\frac{\beta}{1-\alpha-\beta}\ln(s_h) \\ &\quad -\theta\frac{\alpha+\beta}{1-\alpha-\beta}\ln(n+g+\delta) - \theta\ln y(0) \end{aligned} \tag{7}$$

where $\theta = 1 - e^{-\lambda t}$ and λ is the rate of convergence,

$$\frac{d\ln y(t)}{dt} = \lambda[\ln y^* - \ln y(0)]$$

Mankiw, Romer and Weil use their estimates of (7) to examine the issue of convergence, emphasising that it may only be taking place after controlling for the determinants of steady states. Note that, if one is only interested in the question of whether countries are converging or not, this is probably better studied by examining the international income distribution directly (Quah 1993a,b and 1995).[7] However, conditional β-convergence is interesting for a different reason: one can compare the estimated speed of convergence with that predicted by the augmented Solow model.

Unfortunately this comparison is problematic. Although the second set of regressions in MRW allow departures from the steady state, a new difficulty appears. Initial technology differs across countries, so assuming it to be the same will lead to an error term that is positively correlated with initial income. As MRW point out, the coefficient on initial income will then be biased towards zero. One obvious solution is to use panel data to control for

[7] Among other problems, the MRW approach has to face the possibility of 'local' convergence to one of several equilibria, as drawn out in Durlauf and Johnson (1992).

country fixed effects, and Knight et al. (1993) find that interesting results can be obtained in this way. Here, I retain the original MRW approach, but it should be remembered that the speed of convergence implied by a particular regression may be an under-estimate. Later on I consider the problem of measurement error, which could easily bias the convergence rate in the other direction.

First, I consider the question of unconditional β-convergence: do poorer countries grow faster than rich ones, on average? MRW found that the answer was no, except for the OECD sample. My investigation of this question using HCSEs and robust regression estimates (not reported here) indicates that the conclusion is not sensitive to heteroscedasticity or unrepresentative observations. When outliers are excluded, the conclusion for the OECD is strengthened. Initial income is negatively signed and strongly significant. However, countries are within the OECD precisely because they have converged, so this is not an interesting result (De Long 1988). A more detailed study of unconditional convergence would make use of the techniques in Barro and Sala-i-Martin (1992) and Erickson (1993) for dealing with measurement error.

It is perhaps more important to assess the robustness of the conditional convergence results. They have been influential, but may be driven by just a few observations. A test for normality rejects at the 1% level in the intermediate sample, making the use of robust techniques all the more important. In Table 5, I present estimates of MRW's conditional convergence regression made using re-weighted least squares. The results generally bear out the MRW findings. Extending the analysis to the non-OECD samples is interesting. Importantly, the finding of conditional convergence, and its estimated rate, do not depend on the inclusion of the OECD in the sample.

Note, however, that in the two new samples, the population growth variable is insignificant, and the add-up-to-zero coefficient restriction is easily rejected. In other respects the RWLS estimates are satisfactory, with little evidence of heteroscedasticity or non-normality, and insignificant RESET statistics.

Table 5

We can also look at conditional convergence within the quartile samples considered earlier. The RWLS estimates are presented in Table 6. The most interesting aspect of the results is that, although there is evidence of conditional convergence within the poorest and richest samples, there is very little evidence for it in the 2nd and 3rd quartiles. For countries in the middle income range in 1960, it does not appear to be true that relatively poor countries grew faster than others, holding other things constant.

It is clear that cross-section regressions for the whole sample, as carried out by MRW, seem to obscure complex dynamics within the income distribution. These might be best captured by a more direct method, as advocated by Quah, rather than cross-section regressions including initial per capita output. As it stands, it may be that the convergence result for the whole sample is driven by countries at the extremes of the income distribution.

Also worthy of note is the way the coefficient on the investment ratio varies across samples. There is some evidence here for the finding of Durlauf and Johnson (1992), that the underlying technology differs across countries.

Table 6

There is one final issue to consider regarding convergence. It may be that the convergence result is entirely due to measurement error, a point made by Romer (1990). To see this, rearrange (7) to obtain:

$$\ln y(t) = \text{conditioning variables} + (1-\theta)\ln y(0) \tag{8}$$

If initial income is measured with error, while the other variables are correct, then the coefficient on initial income will be biased towards zero. In turn, this will lead to an over-estimate of the speed of convergence. Once one allows for measurement error in at least some of the other variables, the estimated rate of convergence could be biased in either direction.

We can address this question using Klepper's measurement error diagnostics. Table 7 reports extreme lower and upper bounds for the rate of convergence, given bounds on R^{*2}. It is clear that convergence may be so slow in the non-oil and intermediate samples that it is barely taking place at all (and a higher R bound leads to exactly this conclusion). The measurement error problem is potentially too great to draw any firm conclusion. Convergence may not be occurring, or may be rather faster than the 2% figure which has been much quoted in the literature. We certainly cannot take conditional convergence as a stylised fact, or use the estimated speed to draw conclusions about the likely share of capital in the production function. In the presence of measurement error, it is far too unreliably estimated.

It is possible to object that the R bounds imposed on R^{*2} seem rather high, but these reflect the high R^2's obtained using the regression (8) and shown in Table 7. These R bounds are not particularly pessimistic. For instance, to accept them, we must believe that less than 10% of the variation in initial income is due to measurement error. And if the other variables are measured incorrectly, as they are surely are, this condition will be rather more stringent. To obtain narrower bounds on the rate of convergence, I experimented with imposing the restriction that the coefficients on investment, schooling and population growth should add to zero. This did little to narrow the bounds.

Table 7

Overall, the finding that conditional convergence is taking place at a rate of 2% a year should not be trusted. Measurement error makes the estimate imprecise, to the extent that convergence may be barely taking place. In the next two sections I examine other aspects of the augmented Solow model: returns to scale, and the evolution of physical and human capital. I find that MRW are right to conclude that returns to accumulable factors are diminishing. However, in section 5, I show that the assumption they make about the evolution of human capital is rejected by the data.

4 International returns to scale

In arriving at their results, Mankiw, Romer and Weil assume that returns to scale are constant. They estimate that the exponent α on capital in the production function is about 1/3, the usual rule of thumb. Thus, the data provide no evidence of the externalities to physical capital discussed by Romer (1987) and used in several subsequent theoretical models.

However, their estimates may be biased if returns to scale are not constant, an assumption which MRW leave untested. This section of the paper extends the MRW framework to test for scale effects. I start with a production function

$$Y = K^{\alpha} H^{\beta} (AL)^{R-\alpha-\beta} \tag{9}$$

where the parameter R controls the returns to scale. In particular, $R > 1$ corresponds to increasing returns. Using the same method as before, it is possible to derive an equation for aggregate per capita output,

$$\begin{aligned} \ln\left[\frac{Y}{L}\right] &= \text{constant} - \frac{\alpha+\beta}{1-\alpha-\beta}\ln(n+g+\delta) \\ &+ \frac{\alpha}{1-\alpha-\beta}\ln(\frac{I}{Y}) + \frac{\beta}{1-\alpha-\beta}\ln(s_h) \\ &+ \frac{R-1}{1-\alpha-\beta}\ln(L) \end{aligned} \tag{10}$$

where the new term in $ln(L)$ represents a scale effect of the labour force. The model studied previously is clearly the special case in which returns to scale are constant ($R = 1$) and hence the coefficient on $ln(L)$ is zero. With increasing returns to scale, it could also be the case that either α or β now exceed one. However, few would wish to support constant returns to just one factor.

I estimated a restricted version of (10) using the Penn World Tables 5.6 labour force data. Estimation by least trimmed squares gave a negative coefficient on $ln(L)$ for all five samples, suggesting constant or slightly diminishing returns. Using re-weighted least squares suggested that the negative effect is significant at the 5% level only for the intermediate/non-OECD sample. The coefficient implies a value of R of 0.96. Thus, there is no evidence in the cross-country data for aggregate increasing returns. This by itself rules out models like Romer (1986) and Rebelo (1991) in which returns to accumulable factors are assumed to be constant, thereby generating endogenous growth and divergence.

Does the finding of constant or slightly diminishing returns conflict with the finding earlier in the paper, that conditional convergence may not be taking place? Not necessarily. One can imagine many reasons why conditional convergence will not occur, including macroeconomic and political instability, reducing the effectiveness of given investments in physical and human capital.

5 Physical and human capital in the steady state

In this section of the paper, I turn to examining the assumptions made by MRW about the evolution of physical and human capital. Using equations (2) and (5) it is clear that, in the steady state, the following equations should hold regardless of the production function:

$$\ln\left(\frac{K}{L}\right) = \ln\left(\frac{s_k Y}{L}\right) - \ln(n+g+\delta) \tag{11}$$

$$\ln\left(\frac{H}{L}\right) = \ln\left(\frac{s_h Y}{L}\right) - \ln(n+g+\delta) \tag{12}$$

We can use the capital stock data from Benhabib and Spiegel (1994) to test these assumptions directly.[8] The test here is less than perfect: the capital stock estimates are based on a depreciation rate of 7%, whereas $g+\delta$ has been assumed by MRW to be 5%. We will find that, even making this assumption, the capital stock equation is not rejected by the data, once measurement error is allowed for. The equation for the steady-state human capital stock is rather less successful.

The results of estimating (11) are shown in Table 8. It is clear that the estimated coefficients are rather different from plus one and minus one respectively, and the population growth variable $(n+g+\delta)$ performs particularly badly. It is also clear that the problems of the results could be due to measurement error. The errors-in-variables could well be non-classical,

[8] I am grateful to Steve Nickell for this suggestion.

since using 1985 output per capita as a proxy for its steady state value is obviously problematic. Allowing for only classical measurement error, combined with sampling error, equation (11) may hold, but the possible extent of measurement error appears too great to draw strong conclusions. Although the bounds on R^{*2} may seem on the pessimistic side, these reflect the fact that the equation has high explanatory power even when using the badly measured variables.

Table 8

As for equation (12), the results presented in Table 9 show this to be far less satisfactory. Although the R^2 is high for each regression, the coefficient on $\ln(s_h Y/L)$ is never near one, even allowing for mismeasurement and sampling error combined. Making less optimistic assessments of the extent of measurement error does not alter this result. Thus, MRW's basic assumption about the accumulation of human capital is rejected, at least when using their particular proxy for s_h and the human capital stocks from Benhabib and Spiegel (1994). Of course, years of schooling may be a rather poor proxy for human capital, a point made by Mulligan and Sala-i-Martin (1995) based on empirical evidence.

Table 9

6 Conclusions

In a variety of respects the framework introduced by MRW is unsatisfactory. When allowing for measurement error, estimates of the technology parameters are highly imprecise. The equation they present for steady state per capita income performs badly for the OECD, or when the OECD is excluded from the sample. The finding of conditional convergence is also less convincing than it first appears. It may well be due to measurement errors in initial income or the set of conditioning variables. For the same reason, conditional convergence may be rather faster than the much quoted rate of 2% a year. One certainly cannot conclude, as the authors do, that 'countries converge at about the rate the augmented Solow model predicts'.

There are deeper criticisms of the model to be made. The specification for human capital investment allowed MRW to derive an equation for estimation in terms of investment rates. As pointed out by Cohen (1992), it is an ingenious way of estimating a cross-country production function without data on stocks. However, given the poor performance of some of the regressions, particularly for the OECD, it is possible to question the usefulness of the MRW assumption that human capital evolves in the same way as physical capital.

One reason for failure is the obvious inadequacy of the proxy for human capital investment rates. Even allowing for classical measurement error, however, the results of estimating the steady state human capital equation suggest that it is mis-specified. Now that data for human capital stocks are easily available, it seems sensible to use these measures when estimating cross-country production functions for the relevant time periods.

Although MRW's approach is ingenious, it appears that not enough of the underlying assumptions are met to make it a reliable way of inferring technology parameters. In particular, it is difficult to find a good proxy for the rate of investment in human capital. It may also be that technology is better described by an alternative production function. Koop et al. (1995), using a sophisticated Bayesian approach, find that the Cobb-Douglas form is strongly rejected, as is constant returns to scale.

Given that the Cobb-Douglas form is rejected by a more sophisticated analysis, an obvious question is whether the work of MRW falls into the long tradition of spurious fits. Although the convincing argument of Simon (1979) does not apply here, because wages differ widely across countries, it is possible that the function fits well because of the phenomenon noted by Fisher (1992) in simulations. If the wage share is roughly similar across countries for reasons other than a common Cobb-Douglas production function, then the Cobb-Douglas form will often fit well even though it is a poor description of technology.

Overall, MRW seem to be right in their broad conclusion that returns to physical and human capital combined are diminishing. However, the details of their findings should be treated with considerable caution. Measurement error makes the estimates of technology parameters and convergence speeds highly uncertain, while the equation for human capital appears to be fundamentally mis-specified. The model is probably too flawed to take seriously for policy purposes. In particular, one would not want to conclude from it that levels of technology are the same across countries. MRW claim that 'the augmented Solow model provides an almost complete explanation of why some countries are rich and other countries are poor'. The evidence reported here suggests that reality is rather more complex.

This paper is also intended as a demonstration of techniques for robustness testing, some of which have potentially wide application. In particular, Klepper's measurement error diagnostics could usefully be applied to many of the existing results in the growth literature, together with robust estimators like least trimmed squares. Some findings will probably be overturned, others usefully reinforced.

7 Appendix

To identify outliers, each regression in this paper is first estimated by least trimmed squares (LTS), a robust estimator due to Rousseeuw (1984).[9] One problem is that LTS estimates are typically confined to coefficients, and programs are unable to report standard errors and diagnostic statistics. Although standard errors can be obtained by bootstrapping, it is harder to overcome the lack of diagnostics. To remedy this, I use the LTS estimates to classify some of the observations as unrepresentative, and then omit these from an otherwise straightforward OLS regression. This procedure, a simple version of re-weighted least squares (RWLS), is recommended by Rousseeuw and Leroy (1987) among others.

Observations with high residuals are classed as unrepresentative, since they are some distance away from a robustly fitted regression line. This raises the obvious problem that the cut-off for a 'high' residual is inevitably arbitrary. My own solution is to drop observations in such a way that the RWLS coefficient estimates are close to those obtained using least trimmed squares. This seems a reasonable compromise.

However, it is not perfect. We usually think of outliers as affecting the coefficient estimates, but they also have consequences for standard errors. A standard error may be spuriously low because of the presence of just one

[9] Note that this should not be confused with the 'trimmed least squares' estimator studied by Carroll and Ruppert.

observation. The method used in this paper will not detect this unless the coefficient estimates are also affected. For that reason, the standard errors reported in this paper may sometimes be fragile, even though they have been corrected for heteroscedasticity.

A second apparent difficulty is more subtle. If an observation is identified as an outlier within, say, the OECD, we might expect to find it classified as unrepresentative in larger samples. In practice this does not always happen. The fitted model changes depending on the sample, and as it changes, so do the observations identified as unrepresentative. What this suggests is that the underlying production function, if any, is sufficiently different across samples for the classification of outliers to change. This is a problem for the augmented Solow model, rather than my procedure for identifying outliers.

Although my procedure is not perfect, it is quick and easy to implement, and seems far preferable to ignoring the problem altogether. Unless outliers are dealt with in some reasonably objective way, we will be left with fruitless arguments over which observations should be excluded. See Auerbach et al. (1994) and De Long and Summers (1994) for an example of one such debate.

8 Computing Appendix

OLS and re-weighted least squares were carried out using PcGive 8.0, which allows calculation of the jackknife HCSEs. Robust regressions and outlier diagnostics were obtained using the statistics package S-Plus. The package only approximates the LMS and LTS estimators, so some of the results presented here will not be exactly reproducible.

PcGive 8.0, *International Thomson Publishing, Berkshire House, 168-173 High Holborn, London WC1V 7AA.*

S-Plus, *Statsci Division, 1700 Westlake Avenue N., Suite 500, Seattle, WA 98109. Email mktg@statsci.com*

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Table 1
Estimation of the augmented Solow model

Sample	Non-oil	Intermediate	OECD
Observations	98	75	22
R^2	0.79	0.78	0.35
Restriction			
p-value	0.39	0.88	0.97
RESET p-value	0.00	0.22	0.19
Implied α	0.25	0.28	0.13
Implied β	0.38	0.32	0.39

Notes

MacKinnon-White HCSEs in parentheses. The investment and population growth rates are averages for the period 1960-1985. $(g+\delta)$ is assumed to be 0.05. SCHOOL is the average percentage of the working-age population in secondary school for the period 1960-85. The implied values for α and β are calculated from the unrestricted regression, using the relatively weak assumption that $\ln(s_h)$ *is linear in* $\ln(SCHOOL)$. *See footnote 11 of MRW.*

Table 2
Estimation of the augmented Solow model when excluding the OECD
Dependent variable: log GDP per working-age person in 1985

Sample	Non-oil Non-OECD	Intermediate Non-OECD
Observations	76	53
constant	11.5	12.1
	(1.98)	*(2.24)*
ln(I/GDP)	0.58	0.56
	(0.16)	*(0.18)*
ln(n+g+δ)	0.29	0.36
	(0.79)	*(0.91)*
ln(SCHOOL)	0.57	0.67
	(0.08)	*(0.10)*
R^2	0.68	0.67
Restriction p-value	0.04	0.04
RESET p-value	0.01	0.86
Implied α	0.27	0.25
Implied β	0.27	0.30

Notes

MacKinnon-White HCSEs in parentheses. The technology parameters α *and* β *are calculated using the coefficients on ln(I/GDP) and ln(SCHOOL), since the coefficient on* $\ln(n + g + \delta)$ *is poorly determined.*

Table 3
Robust regression estimates, by RWLS
Dependent variable: log GDP per working-age person in 1985

Sample	Non-oil	Intermediate	OECD	Non-oil non-OECD	Intermediate non-OECD
Observations	92	72	19	70	48
constant	7.25	8.13	9.86	13.5	10.7
	(0.93)	*(1.00)*	*(1.52)*	*(1.84)*	*(1.6)*
ln(I/GDP)	0.66	0.79	0.21	0.49	0.66
	(0.11)	*(0.12)*	*(0.29)*	*(0.14)*	*(0.14)*
ln(n+g+δ)	-1.62	-1.41	0.00	1.20	-0.29
	(0.32)	*(0.33)*	*(0.65)*	*(0.74)*	*(0.65)*
ln(SCHOOL)	0.68	0.70	0.02	0.53	0.69
	(0.06)	*(0.08)*	*(0.34)*	*(0.07)*	*(0.08)*
R^2	0.84	0.83	0.04	0.74	0.77
Restriction p-value	0.48	0.85	0.74	0.00	0.12
RESET p-value	0.00	0.46	0.34	0.00	0.82
Implied α	0.28	0.32	0.17	0.24	0.28
Implied β	0.29	0.28	0.02	0.26	0.29

Sample	Unrepresentative observations dropped in RWLS
Non-oil	*Ghana, India, Jamaica, Liberia, Zaire, Zambia*
Intermediate	*India, Jamaica, Zambia*
OECD	*Greece, Portugal, Turkey*
Non-oil, non-OECD	*Hong Kong, Liberia, South Africa, Trinidad, Uruguay, Zaire*
Intermediate, non-OECD	*India, Jamaica, Myanmar, Philippines, Zambia*

Notes

MacKinnon-White HCSEs in parentheses. The technology parameters α and β are calculated using the coefficients on ln(I/GDP) and ln(SCHOOL).

Table 4
Robust regression estimates, by RWLS, stratified sample
Dependent variable: log GDP per working-age person in 1985

Quartile	Poorest	2nd	3rd	Richest
Observations	20	20	18	20
constant	7.64	11.9	8.20	8.39
	(1.70)	*(1.98)*	*(1.02)*	*(1.03)*
ln(I/GDP)	0.18	0.04	0.44	0.32
	(0.08)	*(0.24)*	*(0.20)*	*(0.25)*
ln(n+g+δ)	-0.15	1.17	-0.53	-0.90
	(0.61)	*(0.81)*	*(0.31)*	*(0.28)*
ln(SCHOOL)	0.20	0.40	0.16	0.38
	(0.06)	*(0.12)*	*(0.16)*	*(0.17)*
R^2	0.58	0.58	0.62	0.67
Restriction p-value	0.62	0.07	0.91	0.68
RESET p-value	0.97	0.82	0.80	0.14
Implied α	0.13	0.03	0.28	0.19
Implied β	0.14	0.28	0.10	0.22

Quartile	Unrepresentative observations dropped in RWLS
Poorest	*Botswana, Cameroon, Egypt, Indonesia, Zaire*
2nd	*Brazil, Ghana, Korea, Papua New Guinea, Tunisia*
3rd	*Hong Kong, Jamaica, Japan, Mexico, Singapore, Spain*
Richest	*Argentina, Chile, Ireland, Uruguay*

Notes

MacKinnon-White HCSEs in parentheses. The technology parameters α and β are calculated using the coefficients on ln(I/GDP) and ln(SCHOOL).

Table 5
Tests for conditional convergence
Dependent variable: log difference GDP per working-age person, 1960-1985

Sample	Non-oil	Intermediate	OECD	Non-oil non-OECD	Intermediate non-OECD
Observations	95	68	21	70	50
Estimation	RWLS	RWLS	RWLS	RWLS	RWLS
constant	2.77	3.34	1.34	6.1	6.03
	(0.66)	*(0.68)*	*(1.19)*	*(1.17)*	*(1.26)*
ln(Y60)	-0.29	-0.35	-0.32	-0.38	-0.41
	(0.05)	*(0.05)*	*(0.07)*	*(0.07)*	*(0.08)*
ln(I/GDP)	0.55	0.65	0.13	0.51	0.53
	(0.10)	*(0.08)*	*(0.20)*	*(0.09)*	*(0.11)*
ln(n+g+δ)	-0.59	-0.61	-0.94	0.48	0.31
	(0.21)	*(0.17)*	*(0.29)*	*(0.39)*	*(0.43)*
ln(SCHOOL)	0.20	0.19	0.13	0.20	0.21
	(0.07)	*(0.06)*	*(0.17)*	*(0.06)*	*(0.08)*
R^2	0.54	0.66	0.74	0.57	0.56
Restriction p-value	0.56	0.33	0.05	0.00	0.02
Implied λ	0.014	0.017	0.015	0.019	0.021

Sample	Unrepresentative observations dropped in RWLS
Non-oil	*Hong Kong, Singapore, Zambia*
Intermediate	*Chile, Hong Kong, Jamaica, Korea, Morocco, Singapore, Zambia*
OECD	*Japan*
Non-oil, non-OECD	*Hong Kong, Liberia, Singapore, Somalia, Zaire, Zambia*
Intermediate, non-OECD	*Hong Kong, Singapore, Zambia*

Notes

MacKinnon-White HCSEs in parentheses.

Table 6
Tests for conditional convergence, stratified sample
Dependent variable: log difference GDP per working-age person, 1960-1985

Quartile	Poorest	2nd	3rd	Richest
Observations	21	22	21	21
constant	6.68	-0.04	0.23	2.35
	(2.73)	*(3.92)*	*(2.15)*	*(2.37)*
ln(Y60)	-0.90	0.23	-0.14	-0.37
	(0.29)	*(0.32)*	*(0.19)*	*(0.18)*
ln(I/GDP)	0.26	0.83	0.49	0.56
	(0.12)	*(0.16)*	*(0.21)*	*(0.26)*
ln(n+g+δ)	-0.17	-0.15	-1.06	-0.96
	(0.58)	*(1.00)*	*(0.39)*	*(0.36)*
ln(SCHOOL)	0.11	0.02	0.13	0.14
	(0.13)	*(0.11)*	*(0.18)*	*(0.13)*
R^2	0.57	0.69	0.75	0.84
Restriction				
p-value	0.77	0.41	0.38	0.46
Implied λ	0.092	-	0.006	0.018

Quartile	Unrepresentative observations dropped in RWLS
Poorest	*Botswana, Cameroon, Egypt, Indonesia*
2nd	*Korea, Morocco, Zambia*
3rd	*Hong Kong, Jamaica, Singapore*
Richest	*Argentina, Chile, Uruguay*

Notes

MacKinnon-White HCSEs in parentheses.

Table 7
Convergence rates allowing for measurement error

	Non-oil	Intermediate	OECD	Non-oil non-OECD	Intermediate non-OECD
Lower bound	0.003	0.005	0.015	0.009	0.007
Upper bound	0.067	0.063	0.036	0.029	0.057
R^2	0.91	0.90	0.88	0.84	0.82
R bound	0.95	0.95	0.95	0.87	0.89

Table 8
Testing the evolution of physical capital

Sample	Non-oil	Intermediate	OECD
Observations	92	73	19
Estimation	RWLS	RWLS	RWLS
$\ln(s_k Y/L)$	0.93	0.94	0.90
	(0.03)	*(0.03)*	*(0.07)*
$\ln(n+g+\delta)$	-0.3	-0.09	-0.24
	(0.26)	*(0.22)*	*(0.24)*
R^2	0.95	0.96	0.96
Measurement error analysis			
$\ln(s_k Y/L)$	(0.21,0.98)	(0.23,0.96)	(0.74,0.94)
$\ln(n+g+\delta)$	(-20.8,-0.07)	(-14.1,-0.02)	(-3.3,-0.20)
R bound	1.0	0.975	1.0

Sample	Unrepresentative observations dropped in RWLS
Non-oil	*Angola, Chad, Congo, Egypt, Mozambique, Sudan*
Intermediate	*Argentina, Uruguay*
OECD	*Greece, Japan, Spain*

Notes

A constant was used in the estimated regression, but is omitted from the results since it reflects scaling issues.

Table 9
Testing the evolution of human capital

Sample	Non-oil	Intermediate	OECD
Observations	90	67	19
Estimation	RWLS	RWLS	RWLS
$\ln(s_h Y/L)$	0.29	0.25	0.23
	(0.02)	*(0.02)*	*(0.06)*
$\ln(n+g+\delta)$	-0.08	-0.12	-0.08
	(0.16)	*(0.15)*	*(0.29)*
R^2	0.80	0.83	0.76
Measurement error analysis			
$\ln(s_h Y/L)$	(0.03, 0.30)	(0.02, 0.27)	(0.03, 0.31)
$\ln(n+g+\delta)$	(-9.88,-0.01)	(-5.44, -0.01)	(-7.5, -0.01)
R bound	0.83	0.89	1.0

Sample	Unrepresentative observations dropped in RWLS
Non-oil	*Burkina Faso, Mali, Mozambique, Sierra Leone*
Intermediate	*Ethiopia, Malaysia, Mali, Morocco, Nicaragua, Nigeria*
OECD	*Canada, Denmark*

Notes

A constant was used in the estimated regression, but is omitted from the results since it reflects scaling issues.